literature of that subject by Dr. Charles A. Goessmann, at the present time Director of the Massachusetts Agricultural Experiment Station, but formerly, from 1861 to 1869, chemist to the Onondaga Salt Company, at Syracuse, N.Y. While filling that position he investigated very thoroughly the salt deposits of New York, Michigan, Goderich, Canada, and Petit Anse Island, Louisiana, and his published reports and memoirs (some twenty in number) upon the salines, brines, and mineral springs of the country form, for the period which they cover, a very complete and valuable record of the salt industry in the United States.

Amherst, Mass., May 26. F. TUCKERMAN.

Prof. Greenhill on "Kinematics and Dynamics."

MAY I ask space for a few short comments on Prof. Greenhill's letter in your issue of May 17 (p. 54), so far as it is directed against myself.

(1) The "circumlocutions" referred to are not of my devising, but are current phrases which involve no ambiguity and are useful for avoiding frequent repetition.

(2) It is not true that "although such words as 'a force equal to the weight of the mass of 10 pound weights' do not occur in Prof. MacGregor's book, they are strictly derived from his definitions." According to my definitions, it is the body itself which has weight, not its mass; and the above phrase is therefore meaningless.

(3) Prof. Greenhill has not cited a single instance to justify his charge that I am at variance with my own definition of the weight of a body in the majority of the subsequent examples.

(4) He now seems to admit that in my hydrostatical equations pressure may be expressed in pounds on the square foot, but to claim that it can be done only in a clumsy manner. There is doubtless a certain clumsiness, but it seems to me to be due to the employment of a clumsy set of units.

(5) Your reviewer still demands that I should give the dimensions of the earth, not in terms of the actual metre, but in terms of what the original designers of the metre intended it to be; but he gives no reason for this strange demand.

(6) If the knot is a unit of velocity, the term *knots per hour* is of course redundant. I have always considered it an abbreviation, but have no means at hand of settling the point.

(7) Prof. Greenhill tacitly admits that he was in error in accusing me of misusing the term *elongation*.

(8) He makes no attempt to substantiate his statement that my equations of energy were not expressed in proper form.

(9) He does not answer my question as to which of the most recent treatises on dynamics my treatment of units shows me to have read without profit and discrimination.

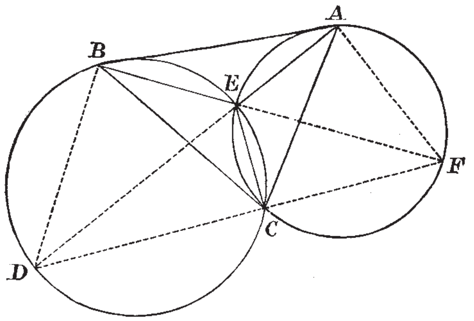
Edinburgh, May 31. J. G. MACGREGOR.

Further Use of Ptolemy's Theorem (Euclid, VI. D.) for a Problem in Maxima and Minima.

To find E within $\triangle$ABC such that

$$AE \sin BEC + BE \sin CEA + CE \sin AEB$$

shall be a maximum.



Keep BEC constant; produce AE to cut circum-circle of BEC (which is then a fixed circle) in D.

Then $\sin BEA = \sin BED = \sin BCD$,
$\sin AEC = \sin CED = \sin CBD$,
and $\sin BEC = \sin BDC$;

$$\therefore \frac{BC}{\sin BEC} = \frac{CD}{\sin AEC} = \frac{DB}{\sin AEB};$$

$$\therefore AE \sin BEC + BE \sin CEA + CE \sin AEB$$

is proportional to

$$AE \,.\, BC + BE \,.\, CD + CE \,.\, BD,$$

and therefore to

$$AE \,.\, BC + ED \,.\, BC. \quad \text{(Eu. VI. D),}$$

which

$$= AD \,.\, BC.$$

For a maximum AE passes through centre of circum-circle of BEC.

Similarly BE passes through centre of circum-circle of CEA. Let it cut it again in F.

$\angle BCE = \angle BDE$,
$= \angle BFA$ in same segment of circle through F, A, B, D,
$= \angle ACE$.

Similarly

AE, BE bisect $\angle$ CAB, ABC.
$\therefore$ E is the in-centre of $\triangle$ABC.

Bedford. E. M. LANGLEY.

Davis's "Biology."

IF I may argue from the contents of Mr. Davis's book, he should be a good judge of what constitutes "falling into a common mistake," and yet I cannot accept his opinion as to my having accomplished this feat. I have refrained from enumerating the common mistakes which his little book contains, but I am not prepared to allow him to lay down the law as to educational methods. In my opinion it is a grievous error to present any subject of study to University students under two aspects, that of "pass" and that of "honours." Whatever is worth doing at all (in academic exercises) is worth doing well, and no regulations sanctioned by any University Senate—however philanthropic, incompetent, and imperial—can make the perennial iteration of the statements in a cram-book concerning six plants and six animals a satisfactory substitute for the study of zoological and botanical science, or anything but a pernicious torturing of the youthful mind. THE REVIEWER.

M. FAYE'S THEORY OF STORMS.[1]

ACCORDING to M. Faye, "There exist in meteorology two theories diametrically opposed—one which considers air-whirls round a vertical axis, including cyclones, typhoons, tornadoes, and waterspouts, to originate in the upper currents of the atmosphere; and the other which considers each of these as the effect of a local rarefaction, giving rise at the surface of the ground, in an atmosphere in a more or less unstable condition, to an ascending current of air, which borrows a gyratory tendency from the rotation of the ground itself." Such is the opening sentence of the pamphlet before us, which embodies a *résumé* of M. Faye's discussions in the French Academy with those who do not accept his peculiar views on the generation of atmospheric disturbances.

M. Faye upholds the former theory with that incisive vigour which characterizes our Gallic neighbours, and attacks the meteorologists with whose writings he is acquainted, beginning with poor Franklin and ending with Sprung in 1885, without mercy, but at the same time without the smallest reference to physics apart from mechanics.

Before pointing out some of the grave errors of fact, as well as theory, into which we deem M. Faye to have fallen, it may be as well to see if we cannot attempt a reconciliation between these two opposite views, which are considered to be prevalent.

To avoid mixing up tornadoes and cyclones, which we hold to be, if not generically, at all events specifically, distinct, let us first consider the former alone. The point that M. Faye insists upon all through is, that these arise solely through inequalities in the upper currents, causing gyration round a vertical axis, which, like a river eddy, is propagated from above downwards, by a *descending motion of the air*. M. Colladon, referring to M. Faye's view, describes the supposed action as "un mouvement tourbillonnaire aérien constituant à son intérieur une trombe *aspiratrice* à mouvement *descendant*." M. Faye, therefore, postulates two points: (1) that the movement commences above; (2) that it is propagated downwards by a descending motion, accompanied by gyration round a vertical axis. The opposite theory, as presented by M. Faye, is the exact inverse of this, since it makes the action (1) commence at the earth's surface; (2) propagate itself upwards; and (3) borrow its gyration from that of the earth. Here, however, we find ourselves distinctly at issue with M. Faye, for we do not believe that the leaders of modern meteorology entertain any such view as the latter. The surface of the earth is the most unlikely birthplace for a tornado, whirlwind, or waterspout. In order to maintain an ascending current, the air must be nearly saturated, and this will generally occur only in and near the lowest cloud stratum. The vertical temperature gradient and disturbances which start the action, will likewise operate most effectively at this level, so that all the conditions which unite to cause a tornado will tend to commence at some distance above the earth's surface. On the question of level, therefore, we may invite M. Faye to agree with us. Then comes the question of the downward propagation.

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[1] "Sur les Tempêtes." Par M. H. Faye. (Paris: Gauthier-Villars, 1887.)

The entire gist of the question appears to us to lie in this downward propagation. The physical theory developed by Ferrel and Sprung makes the action commence in a slight upward motion in unstable air, due to a temperature inequality or some other cause, the only other condition being a gentle gyratory motion relative to some central point, which is never wanting in a cyclonic area. Once the motion is started, and the air which feeds it is nearly or quite saturated, the action will go on and be propagated downwards, not by a descent of the *air*, but by the transference of the physical conditions which favour the continuance and maximum development of the "courant ascendant." The increasing rapidity of gyration of the air as it approaches the axis, however gentle it may be at starting, only allows it to partially feed the initial and continually reproduced vacuum, which is thus compelled to draw its supplies chiefly from the non-gyrating air at the lower end of the aërial shaft. As this is drawn upwards, the centrally aspired surrounding air is made to gyrate more rapidly (partly by the friction of the superjacent rotating layer), and thus the gyratory and other *conditions* are propagated downwards until a balance is struck between supply and demand.

The theory thus sketched may be termed the modern theory of aspiration as applied to tornadoes, and will, we venture to think, be found to meet all M. Faye's objections to the first crude notions which prevailed in past years from a study of a few isolated surface conditions.

Before proceeding to notice the objections which M. Faye brings against the existence of either an upward current or any sort of aspiration in tornadoes, we must first touch upon the cognate question of cyclone generation, which he explains on the same principles;[1] and here, without attempting to give any review of the modern theory, which involves as a primary condition a horizontal temperature anomaly over a considerable area, we may observe that the two main objections brought by M. Faye against the ordinary view of their formation are, (1) that it assumes the existence of centripetal currents, and hence aspiration towards their axes; (2) that it gives no explanation of their movements over the earth's surface.

With respect to the first objection, M. Faye draws attention to a principle which he develops on p. 46, according to which the isobars in the temperate zone do not correctly represent the motions of the air in a cyclone, and says we must look at the isobars in a tropical cyclone if we wish to arrive at correct conclusions.

Here, according to M. Faye (pp. 2 and 46), where "by the *ancient* theory the direction of the wind ought to cut the isobar at an angle of nearly 90°, the angle is sensibly nothing; the pretended centripetal component disappears; and the isobars and the wind arrows display an almost rigorous *circularity*." Again, on p. 12 he ridicules the idea of a barometric gradient in the *tropics*, "where the wind blows precisely *along* the isobars." It is with no desire to indulge in mere polemic that we take up the gauntlet thus thrown down, but the magnificent work of that most careful and renowned inductive meteorologist Prof. Loomis which he has been lately revising, enables us to show most conclusively not only that in the latitude of the Philippines which is nearly the equatorial limit of true cyclones, the direction of the wind in a particularly violent and well observed typhoon cut the isobars right through at the large angle of 62°; but that an extensive comparison of similar conditions, embraced in a large number of violent storms in different latitudes, shows that the angle between the winds and the isobars *increases as it should do according to theory from the poles to the equator*.

The accompanying figure represents the observations accurately, except that the isobars were not as there exactly circular; while the following table shows at a glance how entirely opposed M. Faye's statement is to the true facts, in the very region where he says, "les isobares elles-mêmes dessinent sur le sol comme les flèches du vent un édifice cyclonique *non encore déformé*." We have no hesitation therefore in saying that these observations of Prof. Loomis not only give the death-blow, if one were needed, to the purely circular theory of Reid and Piddington, but constitute a *conclusive* argument against M. Faye's theory of downward gyratory currents and non-aspiration in *cyclones*.

Inclination of the Wind to the Isobars in VIOLENT *Storms.*

	Latitude.		Inclination of wind to isobar.	
	°	′	°	′
Arctic Regions	70	56	28	35
Atlantic Ocean	56	15	30	6
United States	45	0	40	3
India and Bay of Bengal	20	48	57	12
Philippine Islands	14	35	62	12

It is true both from theory and observation that the inclination is less on sea than on land, and usually less as we approach the centre; but the above cases suffice to show the danger which might attend an unmodified adherence to the circular theory, or the rough empirical law of Buys Ballot, which is its practical expression. Dr. Meldrum, F.R.S., of Mauritius, as we have pointed out in a previous article (NATURE, vol. xxvi. p. 31), has frequently exposed the danger of following the purely circular theory, and in a number of the Journal of the Mauritius Meteorological Society for July 1883, he mentions a case in which the captain of the ship *Calédonien* on January 24, 1883, deliberately ran it into the centre of a cyclone by following the circular rules. Fortunately he subsequently became aware of his error, and altered his course just in time to escape the centre.

The second objection brought by M. Faye against the physical theory of cyclones is, that it cannot explain their general motions and course over the earth. We admit that the partial theory, sketched in his opening statement, which he considers to represent the modern meteorological views, could scarcely hope to account for this; but if he will allow the meteorologists to rise with him a few thousand feet above the ground, he will find that the "*drift* theory," of which he appears to regard himself as the discoverer and sole exponent, has for some years been recognized as one of the chief possible causes of the motion of cyclonic systems.

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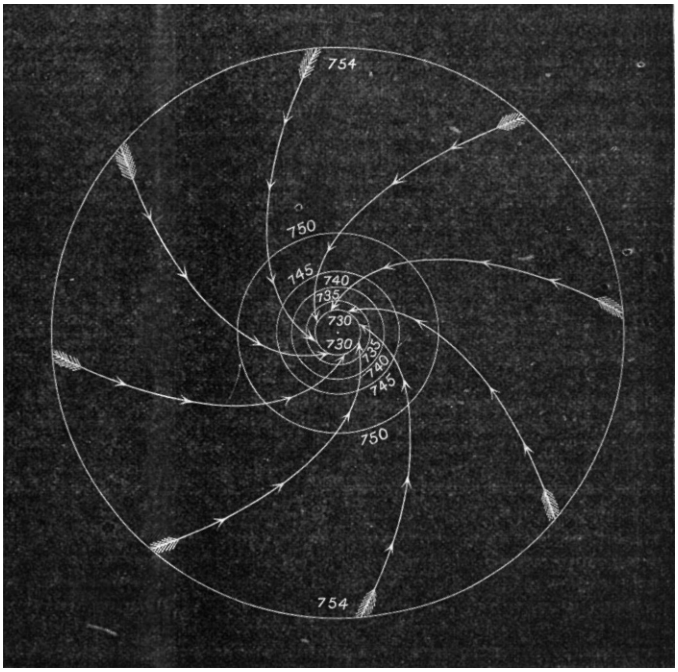
[1] There is no real connecting link between the two, *i.e.* the smaller cyclones do not begin where the larger tornadoes leave off. The average size of 600 tornadoes in the United States was found to be 1085 *yards*. The average size of the cyclones is as many *miles*.

Prof. Ferrel, a representative *deductive* meteorologist, considers the motions of the upper and middle currents to be the *principal* cause of the motion of a cyclone in longitude; its motion in latitude, which is generally towards the poles, being due to the inherent tendency which a mass of fluid gyrating in the same sense as the hemisphere in which it is situated, has to press towards its pole.

Prof. Loomis, an equally representative *inductive* meteorologist, is more cautious; but in his latest work,[1] while admitting the existence of numerous other physical factors to account for the frequently anomalous movements of storm centres—which M. Faye elegantly ignores—he agrees in attributing their *general* directions of translation to the *general* extrinsic movement of the atmosphere at the time, at some height above the surface, in combination with the intrinsic mechanical principle just mentioned.

That these are not the *sole* causes of the motion of cyclones may, however, be freely admitted, and we quite agree with the remark which M. Faye triumphantly quotes in italics from Dr. Sprung's recent "Lehrbuch," on p. 14, viz. that "none of the theories which have been put forward will alone suffice to *completely* explain the motion of translation of cyclones."

Many facts, such as the observed direction of the upper clouds over and surrounding a cyclone, the velocities at the surface in different quadrants, the retardation of the barometric minima at mountain stations, and the frequently small elevation reached by the entire disturbance (not more than 6500 feet according to Loomis)—which are all entirely overlooked by M. Faye—tally more with a species of wave-motion by which the conditions are continually reproduced in a certain direction than with the drift theory, and in any case require other and additional causes for their complete elucidation.



The Manilla cyclone of October 20, 1882. The arrows denote the direction of the wind; the circles denote the isobars at intervals of 5mm.; the inclination of the arrows to the isobars was constant all through, and = 62°·2.

We therefore entirely dissent from M. Faye's dictum that the failure up to date to discover all the causes of the motion of cyclonic areas is to be considered "an irremediable check to their meteorological theory," and we equally fail to recognize how the drift theory as put forward by him strengthens his case in favour of downward motion in tornadoes, or advances our knowledge of cyclone and tornado motion one step beyond the position it has already reached.

To return to tornadoes.

Fully armed with his preconceived theory of gyration, due to inequalities in the velocity of the upper currents, causing a downward motion of air along the axis of the whirl, and completely disregarding all evidence of upward motion or aspiration, M. Faye devotes the main part of his pamphlet to criticizing in turn the various experiments and opinions of MM. Weyher, Colladon, Lasne, and Schwedoff, with the result that he likes none of them, for the very obvious reason that, while they differ from one another in certain points, they all demand aspiration and upward motion along the axis.

We have not space to follow all these attacks in detail, but we venture to think that before attempting to strangle all adverse hypotheses it would have been wise if M. Faye had placed his own theory on a substantial basis of either physical and mechanical principles, or experiment. As it is, the sole foundations he appears to rest upon are (1) the analogy of the river eddy, and (2) the *fancied* absence of all indications of upward aspiration either during or after the passage of a tornado.

Regarding (1) we need only refer to M. Weyher's experiments, which we recently reviewed in NATURE, in order to point out that, by causing rotation at the surface, M. Weyher found himself unable to produce a gyratory system extending downwards into the liquid from the area of rotation. On the other hand, he always found rotation, whether above or below, produce aspiration (accompanied by gyration) *towards* the area initially set in motion. According to these results, therefore, river eddies produced by inequalities in the horizontal flow cannot propagate themselves *below the area of flow disturbance*.

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[1] "Contributions to Meteorology," chap. ii p. 142, revised edition, 1887.

Now it is precisely this very form of river eddy which M. Faye takes as his analogue to the aërial tornado, and it is here that his argument fails; for, while he draws attention to the system of downward motion and gyration in an eddy caused by an outflow through an orifice in the bottom of a vessel containing liquid, where such motion and gyration is evidently caused by the outflow, he is obliged to avoid all reference to outflow at the surface as a cause in the supposed downward atmospheric gyrations. At the same time he imagines that an entirely similar system takes place, in the river, and the atmospheric eddies, as in that produced by efflux, which propagates itself downwards simply through initial rotations taking place in the upper portions, of the liquid in the one case, and of the atmosphere in the other. We have no hesitation in saying that even if such an action were possible, which we strongly doubt, it is in direct opposition to all that we know of tornadoes, either deductively from physical theory, or inductively from the facts which have been recorded up to date.

It would be a laborious, though at the same time distinctly easy, task, to point out the numerous physical facts which accord with the upward aspiration and downward propagation of *conditions* only, and which are utterly opposed to M. Faye's theory of downward motion of the *air*. It would be equally easy to quote numerous observations showing the objective reality, which M. Faye questions, of upward motion in a tornado. Prof. Loomis, for example, who is noted for his caution, relates the following pregnant incident in his own life, in his preface to the revised "Contributions":—"In February 1842 a tornado of unusual violence passed within 20 miles of Hudson. As soon as I received the news, I started out with chain and compass to make a thorough survey of the track, and succeeded to my entire satisfaction. As the tornado passed over a forest of heavy timber, I had the best opportunity to learn the direction of the wind from the prostrate trees; and, by measuring the direction of the trees as they lay piled one upon another, I determined the successive changes in the direction of the wind. *The facts demonstrated incontestably that the movement of the wind was spirally inward and upward, circulating from right to left about the centre of the tornado.* This tornado was but an incident in a great storm which swept over the United States . . ."; and he goes on to say that the results of his subsequent investigation of the latter showed that neither the purely circular theory of Redfield nor the purely inward theory of Espy was correct. The truth, as usual, lay between these two extremes, and the wind, like that shown in the diagram of the Manilla cyclone, really blew in a spiral, curving in towards the centre. Any of the accounts published by the United States Signal Service afford equally strong evidence in favour of both aspiration towards the centres and motion up the axes of the tornadoes. Thus, in the Report furnished by Rev. Charles Brooke, of the West Cambridge tornado of August 22, 1851, the following remark occurs: "No one saw any object driven *downward* by it, but all testify to its taking things *up*" (the italics are in the original); and then follows a list of articles taken *up* and carried, such as boards and slates, to a distance of 3 miles, a large barn 15 feet, a freight-car 60 feet, &c.

Again, in the Official Report of the Iowa and Illinois tornado of May 22, 1873, different witnesses say: "Saw boards whirling round in the funnel." "While the whirlwind was on the river, the water ceased to flow over the dam, although the river at the time was high." "Saw rails *flying out from the summit* [of the column]; an average rail weighs about forty pounds." And we may close the list with one quoted by Ferrel as a well-authenticated case, in the tornado at Mount Carmel, Illinois, June 4, 1877, in which "the spire, vane, and gilded ball of the Methodist church were carried 15 miles to the north-eastward." The whole evidence, in fact, both in tornadoes, and in their milder form of water and sand spouts, is overwhelmingly against M. Faye's views, and in favour of upward motion and aspiration to their very summits.

In his endeavour to bolster up a theory weak at all points, M. Faye seizes upon the well-known phenomenon of the central calm in cyclones, and cites one which occurred in the typhoon at Manilla on October 20, 1882, as proving the general existence of a downward current. In this case, while the thermometer during the first half of the storm marked 24° C., it rose during the passage of the central calm to 31° C., after which it fell again to 24° C. The relative humidity followed analogously inverse changes, falling from 98 to 53—an extraordinary degree of dryness for such a climate. With reference to this circumstance, M. Faye quotes with considerable triumph a remark of Dr. Sprung to the effect that "such a characteristic phenomenon can only be explained by admitting the existence of a descending current at the centre of this cyclone." Locally, and for a short space upwards, there might have been; but these particular features, accompanied by a clearing of the sky, and known as the "eye of the storm," are the exception and not the rule, even in tropical cyclones. It is, moreover, readily seen that if there were a descending current of any extent or velocity in cyclones it would necessitate *an outflow along the surface* for some distance round their centres—a condition utterly opposed to all observation and experience. M. Faye makes one more attempt to support the differential-current-motion hypothesis of tornado and cyclone generation, by referring to certain empirical laws of the relation of the former to the latter disturbances, deduced by Mr. Finley, of the United States Signal Service. For example, (1) the fact that tornadoes are usually found in the south-south-east or dangerous octant of a cyclone; and (2) a law formulated by M. Faye himself, according to which their trajectories, as traced by the areas of destruction, are parallel to those of the cyclones in which they are generated.

The first of these facts has been known for some time, and applies equally to thunderstorms. M. Faye considers it to arise from the air shot *down* from the upper currents reaching its maximum velocity "where the velocity of translation is added to that of rotation," an idea which concentrates in a truly tornadic manner two fundamental errors which pervade his work. Modern investigation has shown that the velocities of rotation and translation in cyclones are quite independent, and is in this matter as far ahead of M. Faye's view as his knowledge of cyclonic systems is superior to that of Franklin, who had no isobaric charts to help him.

Again, the south-east portion of a cyclone is precisely where, according to the corrected theory of aspiration, the conditions are most favourable to the production of local and parasitical disturbances of equilibrium, and since such disturbances take their birth in or just below the cloud-strata, their trajectories will naturally tend to follow the course of these higher strata, which in this part of the cyclone *generally* coincides with that of its translation. The violent motion, moreover, which M. Faye considers to be such an essential primary condition in the generation of tornadoes is by no means necessary, as Prof. James Thomson, among others, has pointed out in a recent paper before the British Association (British Association Reports, 1884, p. 641.)

Besides the objections we have all along pointed out to the existence of the downward current in cyclones, it renders M. Faye perfectly helpless when he contemplates an anticyclone. In the presence of such a formidable foe he is completely disarmed. Here, just where a downward current would come in really useful, he finds he has *used it all up*. All he can say, therefore, is that they have nothing cyclonic about them, which is quite true.

M. Faye concludes by drawing up a list of questions which relate to the phenomena exhibited by cyclones, tornadoes, and waterspouts, and which he considers yet unsolved. Some, doubtless, still await a more complete explanation, but we think the list might be considerably curtailed if M. Faye would descend, if possible, in one of his favourite eddies, and meet the *aspiring* meteorologist half-way. Atmospheric phenomena seldom present themselves in the form of purely mechanical problems. If, as M. Faye says, the question "is not one which can be treated by actual methods of rational mechanics on which everyone can agree," we are equally confident that it is one whose solution cannot be attempted without the aid of rational physics, or without reference to the facts already established by observation.

E. DOUGLAS ARCHIBALD.

THE VISITATION OF THE ROYAL OBSERVATORY.

THE Report of the Astronomer-Royal to the Board of Visitors of the Royal Observatory was read at the annual visitation on June 2.

One of the first points touched on in the Report is the threatened railway invasion of the Observatory.

> The subject of approaching railways has again, after a lapse of many years, engaged our serious attention. Early in March notice was received from the Home Office of a proposal to carry a railway (in extension of the authorized Bexley Heath Railway) in a tunnel across Blackheath, the nearest point being 840 yards from the Observatory. As there was reason to believe that this railway might injuriously affect the Observatory, preliminary observations of the effect produced by trains on the existing Greenwich and Maze Hill Railway were at once commenced, the observations being made on six nights with the transit-circle, and the disturbance in the image of the wires, as seen by reflection from the trough of mercury, being noted. It resulted from these experiments that trains on this railway caused great disturbance during their passage, not only on the section between Greenwich and Maze Hill, the nearest point of which is 570 yards from the transit-circle, but also on the line beyond Greenwich on the London side and beyond Maze Hill on the Woolwich side. The distances of the Greenwich and Maze Hill stations from the Observatory are about 970 and 670 yards respectively. There was also evidence of disturbance caused presumably by trains on the Lewisham, Blackheath, and Charlton line, at a distance of about a mile from the Observatory, but we could only infer the times of passage of these trains from the published time-tables.
>
> In order to establish conclusively the connection between definite disturbances and trains, arrangements were made to note the times of arrival and departure of trains on the Greenwich line and at Blackheath, facilities for doing this having been courteously given by Mr. Myles Fenton, the Manager of the South-Eastern Railway. Observations were made on this plan on five nights, one observer being stationed at the transit-circle to record all disturbances of the reflected image, while another observer travelling up and down the Greenwich line, and a third observer at Blackheath, noted the times of arrival at and departure from the stations. It was found that the disturbance was very great during the passage of trains between Greenwich and Maze Hill, the reflected image being invisible while the train was in the tunnel, at a minimum distance of 570 yards, and that there was considerable disturbance during the passage of trains through the Blackheath-Charlton tunnel, at a distance of a mile, the reflected image becoming occasionally invisible. As the tunnel of the proposed railway would be similar in character to this, but at half the distance, it was concluded that it would cause so great a disturbance as to make delicate observations impossible. On my notifying this to the Admiralty, the Bill was opposed on the part of the Government, and as a consequence of this the clauses authorizing the construction of the railway across Blackheath were abandoned.
>
> I may here mention that the extension of the London, Chatham, and Dover Railway from Blackheath Hill to Greenwich, which was authorized in 1881, is now in course of construction. I hope that, though the terminus of this line is distant only 620 yards from the Observatory, the tremor from trains will not have sufficient time to produce the full accumulated effect in the short interval between Blackheath Hill station and the terminus. But if at any future time a further extension of this line should be proposed, the question would require very careful consideration in the interests of the Royal Observatory.

The following statement shows the number of observations made with the transit-circle in the period of 356 days ending May 10, 1888:—

Transits, the separate limbs being counted as separate observations	5304
Determinations of collimation error	294
Determinations of level error	351
Circle observations	5067
Determinations of nadir point (included in the number of circle observations)	331
Reflection-observations of stars (similarly included) ...	503

> About 350 transits (included in the above number) have been observed with the reversion-prism, to determine personality depending on the direction of motion.
>
> The very bad weather in the first four months of this year has seriously affected the number of observations with the transit-circle.

The total number of observations made with the altazimuth is as follows, the observations having been as usual restricted to the first and last quarters in each lunation, except during the winter, when, in the absence of suitable objects for equatorial observations, the moon was observed throughout the lunation.

Azimuths of the moon and stars	354
Azimuths of the azimuth mark	114
Azimuths of the collimating mark	116
Zenith distances of the moon and stars	209
Zenith distances of the collimating mark......	116

> In consequence of the building operations for the extension of the computing-rooms the collimating mark was dismounted on November 9, and the view of the azimuth mark has been obstructed by the new building from the beginning of December. Since then the collimation and azimuth errors have been determined entirely by observations of high and low stars. It is proposed, when the work on the new building is completed, to select two azimuth marks, one distant and the other sufficiently near to be seen in the foggy weather of the winter months. For distinct vision of the latter a lens of very long focus would be required, and it would thus be available strictly as a collimating mark.

All will regret to hear that no progress has been made since the date of the last Report in the construction of the new 28-inch refractor, owing to difficulty in obtaining the crown disk. The flint disk made by Messrs. Chance seems to be satisfactory, but up to the present neither that firm nor M. Feil's successor has succeeded in making a crown disk.

Attempts have been made to show if anything is gained in sidereal photography by using curved plates. For this purpose a 4-inch photographic object-glass by Dallmeyer, belonging to one of the photoheliographs, was mounted at the end of June in a light wooden tube, and firmly attached to the side of the telescope tube and parallel to it, to carry out experiments on the extent of field available on plane and curved plates respectively, the latter being moulded by Messrs. Chance to a radius of 22 inches, corresponding to the curvature of the field, if the circle of least confusion be taken for the image. We read:—